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THE EGYPTIAN DESERT AND ITS BIRDS.

By Robert Gurney.

The ordinary traveller in Upper Egypt has few opportunities of making acquaintance with the desert which fringes the cultivated strip of land along the Nile, and still less of seeing the true Libyan Desert beyond. An excursion to Sakkara from Gizeh will show him something of the former, but only of its less pleasing features. It is only from Dahchour southwards that the fringing desert can be seen at its best. This fringe varies in width from a few hundred yards to two or three miles, and for the most part consists of stretches of absolutely barren, stony, or sandy ground, as at Abydos ; but in other places there are long sand-dunes covered with tamarisk and coarse wiry grass, with intervening flats, either covered with grass or quite bare sand.

The fringing desert merges into cultivated land, for the Fellah is always ready to sow his lupine crops so near the limit of the inundation that there is usually a debatable ground where stunted lupines struggle for bare existence against the parched sand, and only the pretty little blue Iris thrives.

The Libyan Desert, on the other hand, lying as it does at a higher level, is sharply marked off from the valley of the Nile either by rising abruptly in great limestone cliffs, or in rolling sandy hills, as at Dahchour. Once on the true desert, the sight of a green thing is a rare treat to the eye ; only here and there in a hollow one may come across a patch of plants with succulent leaves or consisting of little more than thorny twigs. For mile upon mile the way leads over a gently undulating expanse of stony desert, where the only living things are sandy-coloured Lizards and Mantids, darting about with prodigious swiftness, and probably feeding on the few minute flies which somehow contrive to find subsistence here. The view is shut in by the gently rising ground, and though every sky-line gives promise of a changing scene, it is a promise which rarely finds fulfilment.

The desert is not, however, by any means all flat or even undulating, but the level plain may give place to more broken ground with bold rocky cliffs, which often show curious effects of the tremendous weathering power of blowing sand and extremes of heat and cold.

Once out on the true desert, practically all bird life is left behind. Here and there, near the edge of it, a little flight of Pipits passes by seeking the patches of vegetation, and birds of strong flight, such as Ravens, Vultures, and Swifts are occasionally seen. The Swift (*Cypselus pallidus*) may be found nesting in rock crevices in the desert some miles south-west of Dahchour. With these exceptions there appear to be no birds resident in these wastes, in spite of the fact that there is a scanty insect fauna subsisting on one knows not what.

It is only in the immediate neighbourhood of the Nile valley that birds are met with in any numbers, the cliffs that bound the valley being a favourite nesting-haunt of many of the birds of prey. These cliffs are composed of limestone, and their line is broken in various places by deep gorges running in for a considerable distance. These narrow gorges are closed in by precipitous walls of rugged rock terraced by ancient water levels, and often honeycombed with water-worn caves and holes of all sizes, many of which have been the abode of Neolithic man or of persecuted Copts, and some of which may even now be the homes of lawless or outlaw Arabs.

The ledges and holes in these steep and almost inaccessible cliffs make admirable nesting-places for birds of prey, or dens for Jackals. It is here that the Griffon and Egyptian Vultures and Bonelli's Eagle nest, and Ravens and Falcons may be seen in most of these gorges in the spring. The Rock Swallow (*Cotile obsoleta*) flits about the bare rocks in some numbers, and now and then Wheatears (*Saxicola monacha*) watch the intruder from the top of a boulder. At night the whistling note of the Barn-Owl and the echoing boom of the Eagle Owl (*Bubo ascalaphus*) may be heard, while Jackals steal out to make their way towards the cultivated land. The crevices harbour countless Bats which, silent during the day, fill the air with their twittering as dusk falls, before, as at a concerted signal, they fly out from every cranny in a cloud.

A curious and rather awe-inspiring noise is sometimes heard in these valleys at sunset. A low rumbling is heard which rises to a vibrating roar reverberating from the hollow rocks and lasting some minutes before it gradually dies away again. Apparently this noise is caused by the slipping of the sand on the steep slopes. At all events, the same noise on a small scale is produced if one slides down one of these sand slopes. As one proceeds, the whole face of the slope begins to move, and gradually a vibrating humming sound rises beneath the sand in a most uncanny way. Apparently these gorges at Abydos were noted in ancient times for mysterious sounds, for Abydos is described in an inscription of about 4000 B.C. as "the region of bellowing."

Between these cliffs and the cultivated land which borders the Nile is, as has been said, a fringe of desert, and here there may be a rather rich bird life. Even here, however, in the dry stony wastes there is little to be seen, but where the moisture and other conditions are suitable for the growth of scanty grass, tamarisks, and other semi-desert plants, a number of birds find a home. It is in these places that the Cream-coloured Courser is met with in small flocks. These birds are not uncommon, though perhaps they are often passed unseen—I have seen a piece of desert covered with their footmarks, and yet have seen nothing of the birds. They are somewhat shy, run with great speed, and readily take wing when pressed. In flight they are very conspicuous, owing to the black markings under their wing.

Another most characteristic bird is the Desert Lark (*Certhilauda alaudipes*), which may be found singly or in pairs along this fringing desert. It is a fearless bird, and delights the traveller by its habit of fluttering up a few yards into the air and then falling headlong to the ground again, singing a sweet little song as it falls. These two are the most striking and interesting birds of this region, but the Sand-Grouse belongs to it too, since it is not a bird of the cultivation and still less of the bare desert. The Sand-Grouse seems to be now a rare bird in middle Egypt. It may be simply bad luck, but in two visits to Egypt, in which I have seen all the varieties of country which Egypt has to offer, I have only once seen Sand-Grouse, though I have occasionally heard them at night.

Of the smaller birds there are the beautiful little Trumpeter Bullfinch (*Erythrospiza githaginea*), and a number of species of Wheatear, Pipit and Lark, which frequent, either constantly or partly, this desert fringe. The Bullfinch is, however, properly a bird of the cultivation, being usually found along the causeways which bound the irrigation basins. The Wheatears are very numerous in Egypt, and while some, such as the Isabelline (*S. isabellina*), the Common, and the Black-eared (*S. albicollis*), seem to prefer the cultivated ground or its immediate edge, all visit the desert and some are rarely seen outside it. These desert forms are *S. deserti* and *S. monacha*. The latter I have only seen in one of the dry valleys penetrating the cliffs, where it apparently had a nest.

Thus it may be said that, so far as Egypt is concerned, the desert itself has no birds resident within it. Such birds as are met with are merely birds of passage, or are making use of its rocks for breeding purposes: but if we extend our definition to include the fringing desert where there is a considerable xerophytic vegetation, there are a number of birds which find here their congenial home.

RELATIONSHIP OF SPECIES.

By H. Panton.

(Concluded from p. 101.)

Let us in our argument term Darwin's slowly evoluting "indefinite variation" a "variant," in opposition to his "definite" or "spontaneous variation," the mutant of De Vries.

The latter great exponent of the mutation theory is of opinion (as above stated) that evolution proceeds from a sudden jump; that a new species is suddenly born; a mutant. What is the difference between the slow changing variant of Darwin and the suddenly evoluted mutant of De Vries? Is it only one of degree? If so, there is no difference. I believe that a mutant is defined as a type that gives inheritable powers, but that a variant does not transmit this power, or hand on its variation to its offspring. This distinction is clear, but is it invariable?

A mutation, I take it, is the result of a power in the germ plasm to give forth suddenly a new form desired by or agreeable to Nature, whether acting through environment or nutriment. It is a natural evoluting process for the manufacture of new forms, and is recognized by Darwin, and although not considered by him to be as effective in evolution as his more graduating variant, is, it is not unreasonable to suppose, apparently like the latter, acted upon by "external conditions affecting the germ-cells." One is therefore forced to the conclusion that a mutant and a variant arise from the same cause.

But some say, as above, that a variant does not transmit its characteristics. Without entering into the argument at all fully, it would appear that there are a large number of cases where variation is hereditary. Nearly all domestic animals are moulded into various forms almost entirely by this means.

And let it not be forgotten that in these domestic forms (or most of them) the selection is not natural but unnatural, and (as in the case of the long-legged game-fowl, which will drop back in one generation to the short-legged type) these variations do not, therefore, become properly fixed but naturally tend to variate back at once to a normal condition. Most of these forms are monstrosities, and only come into being through unnatural interbreeding (with subsequent sterility), against the tendency through natural evolution to revert back from the monstrous to the correct type.

If unnatural evolution can cause a variant to thus pass on its characteristics, it is not perhaps unreasonable to suppose that selection according to the requirements of Nature (whether Lamarckian or Wallacian) may obtain much better results, and pass on its characteristic as unvaryingly as any mutant.

I do not know if Darwin is right in stating that variation gives rise slowly to species. I cannot see any reason why it should not. The matter appears to be by no means settled. Because species have been proved to arise through mutations, this is no evidence that they may not arise by variation.

Some authorities, among them Professor MacBride,* appear by no means to accept the theory of mutation *in toto*, and state that the celebrated example of De Vries, the evening primrose, and on which the chief arguments in favour of his theory are based, may be an introduced form and is strongly suspected of being a hybrid, and thus appear to discredit the theory of evolution by mutation.

As far as I can gather there do appear to be mutants, causing not merely new varieties, such as the white-bellied type of *Mus rattus*, Long-haired Cavies, &c., but even more, even indeed a species (?), at one bound, infertile with the parent form.

Dr. R. E. Lloyd,† writing on the researches of Mr. W. L. Tower regarding the Potato Beetle of America, states: "Here the origin of a species was actually observed (*Leptinotarsa pallida*)"; and later: "A number of *L. multitæniata* were allowed to breed in captivity under conditions which were nearly normal. The great majority of the offspring raised were of the kind *multitæniata*, like their parents, but both *L. rubicunda* and

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* 'Zoology, the Study of Animal Life.'

† 'The Growth of Groups.'

L. melanothorax were born in the same line as sports. . . . The line of *rubicunda*, which was raised directly, both male and female sports of this kind being obtained, was found to be 'almost completely sterile' with the parent species, but quite fertile with specimens of *rubicunda* found at Toluca in natural circumstances."

The noteworthy point in this matter would appear to be the "almost complete sterility" of the sport *rubicunda* with the parent species. It would seem at one jump to have arrived at the "D" stage without any of those intermediate graduating stages, as I have suggested in the Theory of Hybrids. This would appear to give it the appearance of a very extreme mutant, all other mutants that I know of being fertile in the first "A" stage with the parent type. As classed in the "D" stage they become associated with types as divergent as the Horse and Ass, that is, of course, as regards their germ plasm interaction, which, as I have tried to show, by no means is to be gauged by the outward appearance, so far as form is concerned. This would appear to discountenance (as stated before in this paper) if not the theory of hybrids, at any rate the suggestion I have put forward that mutations were confined to the somatic or visible evolution, but that the germinal mutation was more gradual, or rather, the effect on the germ plasm was not marked as regards sterility till a much later stage, *viz.* after two or three mutations, which gives the appearance of a more graduating change in the same. As, however, all these evolutionary matters of varieties, species, and genera, of variants and mutants, appear to be mostly those of degree, it is not incomprehensible to expect more sudden changes at some times than at others, and it would not appear to be any reason that, because some mutations, as far as they are germinal, are sudden, supposition of the general tendency of gradual evolution of germ plasm is unsound, or any theories based on this of no worth whatsoever.

As far as my opinion is of value, this example of the Beetle *L. rubicunda* appears one of the most interesting facts that have been recorded of any mutants, and equalled only by the curious diverse results of those reciprocal hybridizing experiments mentioned by Darwin. Dr. Lloyd makes no special comment on this in his book ('The Growth of Groups'), although his other examples of mutants, which appear to be chiefly based on variations of *Mus rattus*, are, as usual, quite fertile with the parent form.

I am unable to discover where a variant ends and a mutant begins, although one can understand the difference in the main. It appears probable that this ambiguity may be likened to the difference visible in white and negro, but indefinable in their cross-breeds, and also to the graduating difference between species where no line can be drawn.

If we instance Cebus Monkeys, Roan Antelopes, and Red Deer only, I think it will prove a matter of some difficulty to distinguish the mutants from the variants. There seems evidence to show evolution of these and similar wild forms by variation, as in domestic types.

How does the big ankle of the St. Kilda islanders arise? Also the long legs of the Dinka tribes? As mutations? I should imagine not. And are these not acquired? An argument in favour of acquired characters not being passed on is that of the non-transmission of docked tails in Dogs. This, however, is mere mutilation. Nature would hardly inspire the deprived parent, handicapped in its efforts to balance itself, to transmit such a deformity to its unborn young.

Although where Natural Selection gets a chance to work fairly (as conceived theoretically by the Darwinians), one might expect it to do its work and thus obviate the necessity for sudden mutations, it is possible it does not invariably get this opportunity.

The selection that goes on must depend much on chance, namely, disease, famine, battle, murder, and accident, with the best types perhaps killed off. At the best these must be liable to be modified by crossing with bad types, and thus the race in desperation may have to throw off sudden big jumps (mutations) of much better survival value in order to exist; in fact, to make the jump in spite of the selection it has undergone and not through it. Natural Selection must have a better chance of acting on big jumps (mutations) than on slight differences (variations).

Is it not possible that, whereas we may style such evolutions as change of colour from grey to black, absence of horns in species previously horned (individual non-merging characteristics) as mutations, such evolutions as those of racial and merging characteristics one may count to be either variation or mutation, according as the difference is great or small, but with an indefinable separation line? In short, that there is but one evolutionary process at work, and that the rate at which this progresses must of necessity (to be in harmony with a more or less urgent environment) be of varying magnitudes, and hence may lead us into classifying these results as separate phenomena.

It may be of interest to mention the phenomenon of Independent Evolution, a possibility somewhat unconsidered. Dr. Lloyd, discussing the origin of the brown white-bellied type of *Mus rattus*,* surmises that they arise independently all over India, and are not due to one evolution which has migrated. I should imagine that this is a very reasonable deduction from the facts he mentions. I have at present some specimens of this brown type in my possession which were bred by Miss Frances Pitt † from black parents. It is impossible, however, to say if these were an original mutation, or whether mutation occurred previously at the place where the parents were caught (a floating hulk), or in the district in general (Plymouth). Moreover, a seaport is hardly a place to expect isolation. The matter, however, is interesting if only from the fact that this brown type is held to be specifically distinct, under the name *M. alexandrinus*.

A more trustworthy example of types separately evoluted is one given previously in connection with *Leptinotarsa rubicunda*, this type being produced independently from *L. multitæniata*, and, while sterile with its parent form, breeding congenially with a similar identical though unrelated (genealogically) species.

As groups or species appear to arise thus in several places, it follows it is not impossible that genera or families may do likewise, that is, the process might be continued, *pari passu*, to a greater extent, and this might account for curious geographical distribution, such as that of the Tapirs. Platyrrhine and Catarrhine Monkeys may be more widely separated than many imagine, and this, judging from their great dissimilarities, may well be the case.

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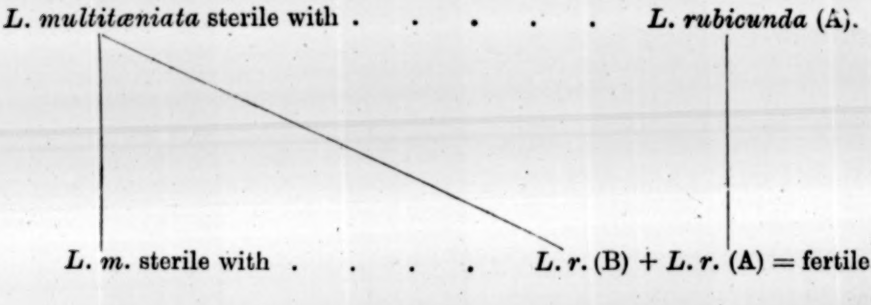
* 'The Growth of Groups.'

† 'National Review,' vol. lix. p. 862.

There appears to be no reason why evolution may not, even does not, act in a similar way on types closely allied or even somewhat divergent, and if the process is repeated (and it would be presumptuous to altogether deny the possibility of such a proceeding), one would get parallelism (and even perhaps convergence) carried out to a far greater extent than appears to be credited or even taken into consideration. In fact, the possibilities of this style of evolution are somewhat staggering, that is, if we can admit that there is perhaps an inherent potentiality in more or less similar forms to be affected by the causes of evolution to similar and somewhat similar degrees.

In this way kinship, or even germ plasm resemblance and fertility, need not depend on recent near blood relationship, and members of the same species might be unrelated in origin; to what degree of remoteness, it would be impossible to surmise.


L. multitæniata sterile with . . . . . *L. rubicunda* (A).
L. m. sterile with . . . . *L. r.* (B) + *L. r.* (A) = fertile.

In this way we have seen how one strain (B) of *L. rubicunda* is produced from *L. multitæniata* and sterile (partly) with this form, and on account of this, and also because the former is fertile with another independently evoluted strain (A) of *L. rubicunda*, it may be assumed this "A" strain is also sterile (partly) with *L. multitæniata* (this should be tested).

We have thus a case of convergence in *L. multitæniata* producing a form of *L. rubicunda* indistinguishable from and fertile with the original *L. rubicunda* strain. If this breeding had not taken place under observation in partial confinement, who would have felt inclined to admit the possibility of a genealogical tree similar to the above?

I have mentioned above that the supposed fertility between

E. zebra and *E. asinus* suggests that the former is more nearly related to the latter than to the other Zebras.

Some years ago it was pointed out by Mr. R. I. Pocock* that there were certain markings which are possessed by all the striped forms, generally referred to as Zebras, which are not found in any other of the *Equidæ*.

It has struck me that some such explanation as the following might be as permissible as such alternatives as that of these markings being caused by some peculiarities of the environment (such as are considered by some to induce these parallel cases), or that of being caused by mimicry.

Let us assume instead an evolution of the more specialized (?) rock-haunting forms, *E. zebra* and *E. asinus*, from intermediate types of Equines. Let us admit it as possible, if not probable, that *E. zebra* is a specialized form descended from some ancestral intermediate type, with medium hoofs, ears, tail, mane, &c., akin to the Quaggas, and with stripes and markings peculiar to Zebras in general. That in the course of this evolution there were independent strains from the ancestral type making towards the same goal; that is, that all the present tribe of *E. zebra* are not absolutely descended from one original Adam and Eve pair. This assumption would not be greatly in excess (as probability goes) of the facts noted in regard to the Beetle *Leptinotarsa*.

We shall here admit α and β strains to be one and the same species, for it is impossible to tell the difference between them, or which individuals belong to which strain, or even if there are two strains, or two hundred, or perhaps, in conformity with general opinion, only one. It is also quite easy to admit the probability of sterile hybrids between the specialized and ancestral forms.

Admitting, therefore, independent evolution of this description, we may proceed a stage further and suppose the presence, contemporaneously with the ancestral Quagga-like type, of another intermediate form, also with medium hoofs, ears, tail, and mane, but of uniform coloration and akin to the Onagers; the above two ancestral types being sufficiently allied and congenial to each other to produce fertile hybrids.

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* 'Field.'

What is to prevent an evolution of this Onager-like type, under the influence of the same evolutionary duction that has been acting on the striped Zebra line, to specialize into *E. asinus*?

Condition 1 to be a physiological state, in which this condition is so congenial to the germ plasm of nearly related forms passing through it, that they, while under its influence, would so retain their relationship and germ plasm affinity that fertile hybrids would be produced between them.

Condition 2 is a physiological state after the evolution of these forms from Condition 1. One inducing a physiological condition congenial and necessary, let us say, to rock-haunting species, and one causing them to become infertile with the ancestral types left behind in a different physiological environment. These specialized (?) forms resuming their congeniality *inter se*, and again producing fertile young on their arrival at Condition 2, and after their passage through the evolutionary period (congeneric barrier).

We now arrive at the result that, speaking congenially (or even specifically or generically), *E. zebra* is nearly akin to *E. asinus*, and remote from the ancestral types, although retaining the Zebra markings characteristic of this type; and in so much this would meet facts similar to those put forward by Mr. Pocock.

I have said in the above sentence "even specifically or generically" we have admitted we cannot separate the "α" Zebra strain from the "β" strain, notwithstanding that they do not connect genealogically except through the ancestral Quagga-like type, then why should we disconnect *E. asinus* from them? Are we to separate the *E. asinus* type on account of a different coloration from the Zebra type, and classify it with *E. onager*, while we associate the "α" and "β" Zebra strains in one species, although they (the latter) are practically almost as distinct from each other as from the *asinus* strain? Is *E. asinus* to be taken from the evolutionary condition No. 2 and classed with animals of the evolutionary condition No. 1, when it has arrived at the same stage in evolution similar and congenial to the *E. zebra* strains?

I do not wish to imply here, by using the species I have in this illustration, that these Equines have been subjected to or have gone through any such evolution as that portrayed above; I use them because they seem very aptly to illustrate a process which is not perhaps an impossibility in evolution, and that in some such manner as this it is perhaps permissible to look for the rather baffling discrepancies that arise in estimating nearness of relationship, and which hypothesis may be suggested by such possibilities as those of independent evolution.

There is a tendency to bring forward the discoveries of Mendel as refuting those of Darwin. I do not see any reason for this. One argument (against the latter) appears to be that, while the theory of Natural Selection accounts for the acceptance of the most adaptive form, there is no conceptive building-up principle or resultant in this to account for the new forms being brought into being. Darwin, however, definitely states that the external conditions affect the germ-cells slightly and affect their hereditary potentialities: this very feasible and likely supposition thus clearly producing variations, even if there were no Natural Selection to single out the best, consolidate and mould them.

This view of the case is very clearly put by Professor MacBride, who points out[*] that Mendel himself refused to investigate those characters which were of a "more or less" description.

It would appear, however, that while Mendel's work consisted in showing how two different races with different characteristics may, when crossed, transmit these qualities in different ratios, and how even new true breeding species or forms might be evolved by crossing these forms, certain followers of Mendel assert that these forms demonstrate the evolution of species. This, as Professor MacBride states, "brings us up against a blank wall"; as very evidently these cross races must be evolved from two already evoluted races.

There may be, of course, some examples of a new united race forming in nature as in artificial types, such as wheat (but then it must be from already evoluted types), but one may be excused if one imagines these instances to be rare, at least in the higher forms.

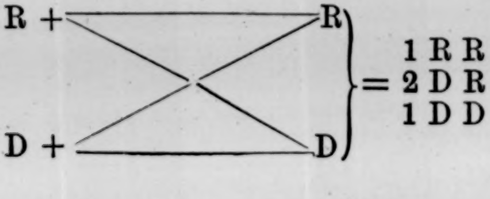
It would seem that the new species of this description are

[*] 'Zoology, the Study of Animal Life,' pp. 69–72.

very generally an artificially produced race of bastards (an unstaple race suggestive of and a precursor of the infertility of more evoluted crossings), certain forms of which are continually endeavouring to break back into the two parent types; these latter, indeed, being the only staple, true-breeding young of these mongrels. For instance, in what is styled the roan "hybrid" (in cattle), in the first mongrel generation, while all the offspring are of mixed roan type, at the fourth generation the mixed hybrids (roans) have decreased to 1 in 6 as against the pure ancestral types (reds and whites), while in the fifth generation they are 1 in 13, and in the sixth only 1 in 27, and so on. Mendelism in this appears only to no more than touch on the first signs, the fringe of evolution, and (to use a chemical simile) while the extreme form of species, such as Dog and Fox, may be looked upon as different chemical compounds, Mendelian new species may be said in comparison to be no better than mere mechanical mixtures; while as regards the more distinct forms, such as new wheats, as bred from two divergent natural types, there appears only an artificial bastard race which, contrasted with the naturally evoluted parents, can by no means be considered examples of evolution.

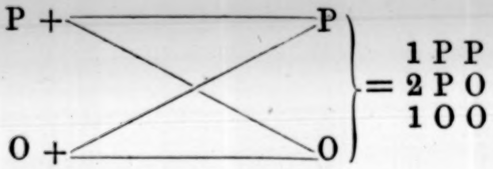
Where do we find in Nature any example amongst mammals of species thus evolving from any such process as the experiments of Mendel? We cannot definitely say. Take that gradually merging group, the Antelopes. Where is the Mendelian new form? Possibly only in the mongrel Cobus at the Zoo, and this perhaps not a true "Mendelian" hybrid. Moreover, those races which lend themselves to Mendelian experiment are chiefly those that (as shown by certain biologists) are deficient in some natural character. Thus the albino Mouse or Rabbit is deficient in colour, the dwarf pea is deficient in size, and so on. And thus, instead of the dominant and recessive characters of Mendel shown by diagram to be as follows:—

R R + D D = 2 R D and



the terms "present" and "absent" are often used, as might be shown by P and O, thus:—

$PP + OO = 2PO$ and



Several investigators have pointed out a difference in crossing various types; that in some instances the characters follow the usual Mendelian laws and do not blend but segregate, while in other crosses the characters blend (as in equine Mules).

Whether non-segregating crosses will prove amenable to Mendelian interpretation appears a matter of considerable doubt. At present this difference in result appears of great importance, and it has been suggested that it is the non-segregating characters that one must look to for the conception of species (De Vries *). I do not gather, however, that this theory is accepted by Mendelians in general.

The idea occurs to one that the same Mendelian factors are carried through the generations irrespective of the fact that, let us say, the Horse is becoming more "horsey," or the Ass more asinine; or whether it is a Horse or an Ass germ that carries them. The colour of an equine Mule, for instance, is perhaps acted on just as truly by the divergent parent factors as would be the case were these combining factors both from Horse (or Ass) parents; they also no doubt affect such qualities as immunity to certain diseases, sex determination, &c. There appears, however, no factor for "horseness" or "assness," and one therefore inclines to the conclusion that, as Mendelism does not explain the intermediate forms resulting from the interbreeding of two diverse species, it supplies no evidence of the evolution of these from a common ancestor. Should the formula for such crosses be discovered (if there be one), it would hardly explain evolution.

In reading the works of Professor Bateson † or Professor

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* 'Mutationstheorie.'

† 'Mendel's Principles of Heredity.'

Punnett,* one appears to get but little information on this point, that is, as regards the why and wherefore, as one does from Darwinism. Neither does one discover much regarding such matters as hybridism and relationship; that is, touching such details as I have raised in this paper.

Considering the abstruse cases which have, when brought forward as antagonistic to Mendelism, proved amenable to its laws, one hesitates to dogmatise unduly; still, there are cases which Mendelism apparently does not solve, and it is possible that, were experiments more general between divergent natural species (animals) instead of between domestic varieties, these incomprehensible (?) cases would be largely increased. As the matter is of considerable importance, I will quote some cases from the writings of Bateson.†

De Vries is stated to have found out that *Œnothera muricata* + *O. biennis* gives hybrids breeding true to the intermediate forms of the first crossed generation. Professor Bateson comments as follows ‡ :--

" The hybrids were partially sterile in a high degree, but the subsequent generations raised from them showed no definite departure from the F_1 type. The evidence as it stands must be taken as constituting a definite exception " [to segregation]. " Nevertheless, in view of the great sterility exhibited by the hybrids, and the fact that all that we know of the *Œnothera* crosses points to the existence of very unusual features in their genetic physiology, the significance of this curious observation is still somewhat problematical."

The fact that the hybrids are partially sterile is to my mind very suggestive, as it will probably prove to be to those who agree with my argument.

Other instances similar or somewhat similar to the above are found in *Homo* (Mulatto), *Lepus* (Lop-eared Rabbit), *Acidalia* (Moth), *Pararge* (Butterfly), and *Anemone* (plant). Want of space must preclude my referring to these cases at any length, except to state that they appear to support my argument.

It is regrettable that more experiments are not tried with the higher animals (natural species). Most valuable animals for a test of non-segregating characters (and other matters besides) would probably be the hybrid Bears mentioned before in this article (hybrid Brown Polar, London Zoological Society).

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* 'Mendelism.'

† 'Mendel's Principles of Heredity.'

‡ *Loc. cit.* p. 250.

It can hardly be denied that the presence of intermediate characters in divergent crosses (extreme species) suggests intermediate, and therefore merging, evolution, such as I have found evidence of in the results of hybridization which I have given on p. 28.

We are obliged to admit two cardinal points :—

(1) Mendelians offer no explanation as to these intermediate characters.

Thus: "In the phenomena of hybridization between the races of mankind the facts (fairly well-authenticated) must be taken as pointing at all events to the existence of special features in them." *

And, again, as regards dimorphisms in Lepidoptera (p. 257): "The only other suggestion" [to the presence of factors] "which can be made is that these distinctions are ultimately referable to the effects of external conditions."

And, again (p. 285): "Of the consequences of specific crosses in the stricter sense little is known, and no case has been fully explored. But before any decision at all is pronounced or even contemplated, the laws which govern the incidents of sterility must be most carefully determined."

On the other hand, we have the definite opinion on p. 289: "The conception of Evolution as proceeding through the gradual transformation of masses of individuals by the accumulation of inpalpable changes is one that the study of genetics shows immediately to be false. Once for all, that burden so gratuitously undertaken in ignorance of genetic physiology by the evolutionist of the last century may be cast into oblivion."

This brings us up against our second cardinal point :—

(2) That we have the evidence of the development of many extreme types of the same domestic (and even wild) animal brought about by gradual divergence quite unconformable with any system of factors, as now propounded.

If I may make the suggestion, it appears to me that we must,

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* 'Mendel's Principles of Heredity,' p. 257.

in order to reconcile these apparent contradictions (Mendelian and Darwinian), recognize that we are arguing about two entirely different matters, namely (*a*) the germ plasm evolution; and (*b*) the ornamental accompaniments of that evolution—the colour, hair, &c., of Mendelian investigators.

We must cast aside all endeavour to attach to such superficialities the index of true species (extreme). If we recognize the essential evolution as that of essence and germ, the difficulty of conflicting theories fades into insignificance.

Mendelians appear to suggest (or this is how I understand it) that the Horse and Ass contracting germs should contain certain factors in their Mules, some to account for the Horse points, and some for the Ass points. The inference from this is that otherwise they are to be held inalienable. To limit our view to the differences of factors alone appears to me to obscure the issue. The two combining entities must be considered as of quite divergent composition.

If we disregard this search for problematical factors, we can regard the cells (in all intermediate crosses) as exactly what they very probably are, namely, a composition of equal properties of both parents, entities of mingled physiological combination, and therefore of intermediate aspect. There is a hybrid Zebra-Ass at Regent's Park, and if this animal proves fertile, I cannot imagine that she would throw (to a mate of similar breeding) an Ass, a Zebra, and two impure somethings. Why should she? The answer appears obvious.

Let us assume that there be factors (bio-chemical), as may indeed be the case—there appears no reason why they should of necessity be segregating ones. If such a result is to be proved in all sperms, with no inherent specific diversity, there should apparently be no limit to any monstrous hybrid of any parentage, or how would sterility be accounted for? Chemistry of the organisms has unknown possibilities; it would appear hardly capable, however, of turning an animal whose organism is half Zebra into a homogeneous Ass.

Segregating characters are of necessity more incomprehensible than intermediate ones; there seems no need, therefore, for any assumption that the latter must be conformable to the same biological laws. The "presence and absence" theory seems quite inapplicable to the evoluted and diverse types of extreme species. One appears able to detect, however, even in extreme crosses, features which suggest Mendelian interpretation, such as an assumption of the hybrid of the *outward appearance* (possibly nothing more) of the male parent. Thus Horse ♂ Ass ♀ cross appears about ¾ Horse, while Ass ♂ Horse ♀ cross seems, on the other hand, approximately ¾ Ass.

This is a well-known characteristic, but it is impossible to say what would eventuate if these forms were fertile. As these Mules are quite common, the matter would no doubt have been investigated but for their sterility. This, however, is unimportant, as there appear to be forms showing this tendency which are fertile, such as the hybrid Bears, Zebra-Ass, &c., mentioned above. It would almost appear that the male carries some factor for "form" which is perhaps absent in the female. The matter should be investigated by zoological societies, as mostly possessing the means and opportunities to carry on these experiments.

Darwin has been criticized for following a false track in advocating the theory of gradual divergence, and it is suggested that, had he known of Mendel's work, he might have altered his opinion. This is mere supposition.

Even if Darwin gave less heed to mutation than variation, the distinction is not important to my argument: a stair is a gradually ascending (or descending) plane, and our ascent is gradual even if, at intervals, we take two or three steps at a time.

Darwinism is said to have overshadowed and hidden Mendelism; the position now appears to be somewhat reversed.

When one remembers that the evolutionary idea was not wanting in the days of Aristotle and the ancients, and that many of the later discoveries are unconsciously fixed in the mind of the cowherd in the barton, it appears possible that the tendency is to apportion too much credit to later investigators than to those of former times.

My own deductions may be held to favour surmise to the detriment of systematism. Both tendencies carried to excess are no doubt undesirable.

While I venture to hope that the examples I have given may protect me to a certain extent from being held to be merely a theorist, one might claim to find some support from the words of Vigors,* who, criticising the other extremist, writes:—

"It is upon the labours of man that he dwells, and not on the works of Creation. He dwindles, as it were, into a mere compositor of the volume of Nature, artificially putting together the symbolic words that should stand for ideas, while the ideas themselves in their true spirit and meaning escape him. And thus the exertions, which, properly directed, might have tended to explain the laws and elucidate the operations of Nature, which might have been devoted to a study purely intellectual, are lost in a pursuit which is strictly and exclusively mechanical."

I have personally not been able to get beyond the Unknown Power, be it natural or supernatural, in the evolution of species any more than in the evolution of worlds. The chief matters that seem to be definite and not pure supposition are the interesting facts concerning the microscopic discoveries that have been made in the composition of the germ cell as regards its nucleus and chronosmomes, and it is perhaps to further discovery in this direction that one might expect more facts to come to light bearing on the problem of the Relationship of Species.

SUMMARY.

1. That there are practically two evolutions:—

    (*a*) Visible and Corporeal.
    (*b*) Invisible and Essential.

And while they are both never out of harmony, we are by no means always able to estimate from the visible aspect of the former (*a*) the degree of evolutionary progress attained by the latter (*b*).

2. That the blood and germ affinity may change without loss of structural resemblance (*a*).

3. That the form of animals may change without loss of relationship (*b*) [blood and germ affinity].

4. That where these two evolutions cause apparent anomalies, or point to opposite conclusions, habits which are congenerous

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* Quotation from 'Cassell's Natural History.'

in any of the forms are of value in probably showing the true relationship, and might be considered important enough to turn the scale in this direction.

5. That parallelism and convergence may take place as regards structure (*a*).

6. But that whether these two tendencies of evolution take place as regards the bringing together of divergent germ plasms (*b*) is perhaps not quite so apparent.

7. That the facts brought to light concerning the separate evolution of similar forms suggest parallelism to a wider extent than has been credited, and may alter our conception (genealogically) of the Relationship of Species.

8. That, while segregating characters in hybrids appear of varietal value, intermediate ones suggest evolution of species.

THE PHARYNGEAL TEETH OF FISHES.

By Colonel C. E. Shepherd (Indian Army).

(Continued from vol. xvii. p. 389.)

Amiidæ.

Amia calva (the Bowfin), found in the rivers and lakes of North America. This fish is one of those that makes a rude nest amidst the roots of swamp vegetation, in which its eggs are deposited, and over which the male fish keeps guard. It has fourteen short upstanding gill-rakers on the first cerato-hypobranchial arch, in length the longest is about half the depth of the gill-lamina below it. There is a set of similar gill-rakers on the inner side of this branchial arch. There are three similar gill-rakers on the first epibranchial, with two rudimentary ones at the top. The gill-rakers on the cerato-hypobranchial arch decrease in size till they become very small at the end near the tongue. The other branchial arches all carry a double row of similar but smaller gill-rakers, and all the gill-rakers bear minute teeth. For the upper pharyngeal teeth there are two very small points at the top of the second epibranchial which carry defined teeth and below these there is a little roughness on the surface of the epibranchial. The third and fourth epibranchials support a triangular shield with the apex upwards, of cardiform teeth. The lower pharyngeal teeth are set in two small elongated triangles.

Lepidosteidæ.

Lepidosteus osseus. A freshwater fish of North America, colloquially known as the "Long-nosed Gar Pike," a voracious fish preying largely on smaller fishes. It has eleven flat tubercle gill-rakers on the first cerato-hypobranchial with six on its epibranchial; and the same kind on the outer sides with smaller and more numerous ones on the inner sides of the second and third arches. The inner side of the first and the outer side of the fourth arch have tubercles also. Along the top of the hypobranchials where they join the basibranchials and for a short distance there are minute teeth on the four arches, they are so small as to be discovered more by the feel of resistance they give than by sight. At the top of the second epibranchial there is a small group of minute teeth representing the upper pharyngeal teeth.

ELOPIDÆ.

Megalops atlanticus (the Tarpon) frequents the Atlantic Ocean along the south-east coasts of North America and the West Indies. A fish much prized by wealthy fishermen who can afford to go across the ocean for the pleasure and excitement of catching it. It is called the "Cuffum" at Demerara. This fish has forty-four long horny gill-rakers on the first cerato-hypobranchial arch, that decrease in size very gradually as they approach the basibranchials. The longer ones near the angle are about a third longer than the gill-laminæ below them. There are twenty-two on the first epibranchial. All the gill-rakers are toothed on the inner side so that they cling to the finger if it is brought against them from base to tip. The outer sides of the second, third, and fourth arches carry horny gill-rakers of the same pattern as those on the first arch but much shorter. The inner margins of the branchial arches have no gill-rakers except the fourth which has short tubercles which fit into larger tubercles on the outer side of the fifth arch. The top of the branchial arches, and the top of the basibranchials, and the epibranchials and lower pharyngeal bones are all covered with villiform teeth feeling like moderately coarse sandpaper, and these extend on to the tongue.

Elops saurus, found in tropical seas; the specimen examined came from Madras. This fish has twelve long horny gill-rakers on the outer side of the first cerato-hypobranchial, the longest about equal to the depth of the gill-laminæ below it, with eight long horny gill-rakers on the first epibranchial arch. There are no teeth on these gill-rakers. The outer sides of the second, third, and fourth arches carry horny gill-rakers, decreasing in size as they belong to the inner arches. The inner sides of the three inner arches also have small tubercles rather distantly spaced. The head of the second epibranchial and the upper pharyngeal bones are covered with minute cardiform teeth. The lower pharyngeal bones have similar minute teeth. There is a rough surface on the basibranchials and tongue.

ALBULIDÆ.

Albula conorhynchus, a fish that swims in tropical seas; the specimen obtained came from Java. It has eight tubercle gill-rakers on the first cerato-hypobranchial and four on the first epibranchial. Similar tubercles are on the inner and outer sides of the other arches that bear gills, all these tubercles bear teeth. There are minute teeth in two patches on the heads of the third and fourth epibranchials that run down on the limb of the third and are confluent at the upper part of the arches. The patch on the fourth is fairly large. There are a few minute teeth for the lower pharyngeals. Minute cardiform teeth are found running from the base of the tongue along the surface of the basibranchials, they seem fixed to the skin, some of them have been rubbed off. The hinder larger part of the tongue has granular teeth on it that show very clearly. Fig. I., 1.

NOTOPTERIDÆ.

Notopterus afer from the Gambia River has seven long horny gill-rakers on the first cerato-hypobranchial arch with one rudimentary one; these are toothed; there are three on the epibranchial. The longest is about one and a quarter times the length of the gill-lamina below it. There are short horny gill-rakers on the other arches. The upper pharyngeal teeth consist of a small group on the head of the fourth epibranchial of villiform teeth. The lower pharyngeal teeth are in two small groups of similar teeth. A long broad plate of minute cardiform teeth extends from the base of the tongue to the junction of the third arch with the basibranchials. The tongue is provided at its extremity with a recurved sharp conical tooth, there is a longitudinal groove in the mucous membrane of the tongue with six side grooves on each side, in each of the side grooves is a similar tooth, and by pressing this mucous membrane down, the teeth can be easily seen. In the illustration this membrane has been cut away to expose the teeth. Fig. I., 2. There is also a group of teeth on the parasphenoid bone.

Notopterus chitala, a fish found in the rivers of India, has eight long horny gill-rakers with two rudimentary ones on the first cerato-hypobranchial and three on its epibranchial. They all bear teeth. The longest equals in its length the depth of the gill-laminæ below it. The one at the angle is very short. Similar but smaller gill-rakers are on the other arches, they get shorter as they belong to the more inward arches. The insides of the arches have short upstanding gill-rakers fitting into the opposite ones and making a good filter. The upper pharyngeal teeth are represented by a small group of minute cardiform teeth on the heads of the third and fourth epibranchials. The lower pharyngeal teeth are in two long plates of minute cardiform teeth. A long plate of minute villiform teeth covers the forward basibranchials. There are five backwardly curved teeth along each edge of the tongue, with one at the tip with sharp points. This one and the next two on each side are strong, the other three each side are smaller. At the base of the tongue and lying between the marginal is a group of short but strong

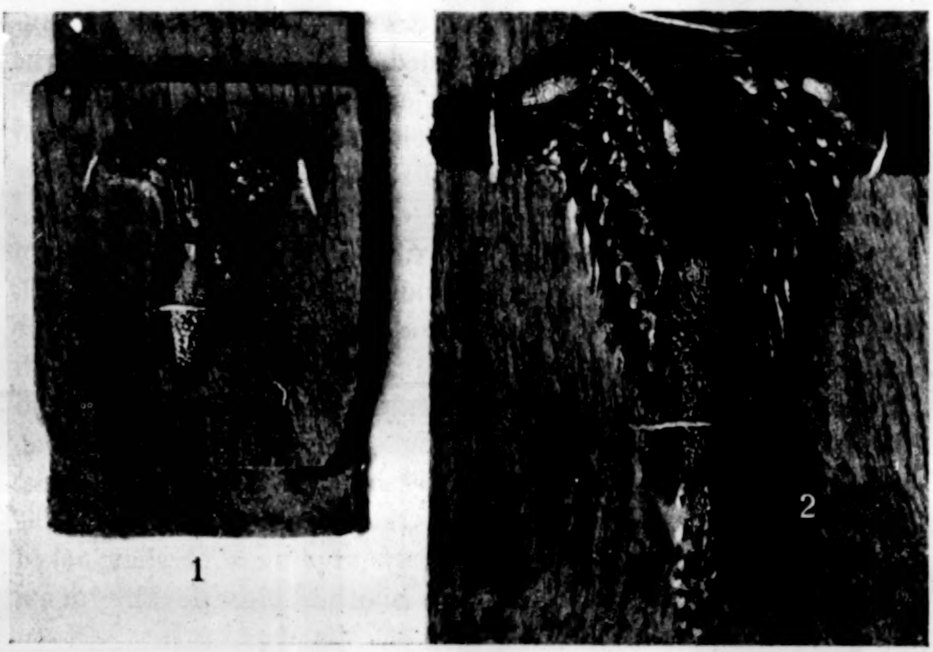

FIG. I.

1.—ALBULA CONORHYNCHUS. 2.—NOTOPTERUS AFER.

cardiform teeth. The stomach held scales and vertebræ of some fish that had been eaten. No teeth on the parasphenoid. A deep cleft in mucous lining of palate.

Notopterus kapirat, also an Indian river fish, has nine long horny gill-rakers on the cerato-hypo portion of the first branchial arch. The one at the angle is short, about one half the length of the next one to it, which is one of the longest and is one and a half times in length of the depth of the gill-lamina below it. There are three gill-rakers on the first epibranchial. The gill-rakers of the first arch are toothed. The other gill-rakers are short, stout, blunt-ended on the outer sides of the other arches, with smaller blunt-ended ones on the insides. None of them bear any teeth. They fit into each other and form a good filter. On the head of the fourth epibranchial some minute teeth can be felt forming the upper pharyngeal teeth. No lower pharyngeal teeth could be seen or felt. Forward from the junction of the third hypobranchial with the basibranchial, to the base of the tongue, there is a long plate which covers the forward basibranchials bearing minute teeth ; these engage with an elongated group of strong conical teeth on the parasphenoid bone. Five strong curved teeth line each margin of the tongue, with one at the tip. There are no teeth at the root of the tongue, as in *N. chitala.* The stomach of the specimen examined was full of food. Amongst it was a fairly large piece of what looked like the shell of a crustacean, and another piece looking like a fragment of some insect.

CHIROCENTRIDÆ.

Chirocentrus dorab has fifteen long, thin, horny gill-rakers on the first cerato-hypobranchial arch, which are toothed. The longest, the one at the angle and the one next to it, are about equal to the depth of the gill-laminæ below them; these gill-rakers decrease regularly in size as they get nearer the tongue. There are four similar but smaller gill-rakers on the first epibranchial arch. On the inner sides of the first and of the other arches there are minute teeth. The outer side of the second arch has shorter and flatter horny gill-rakers than those on the first arch; these are also denticulated. The third and fourth arches are bare of gill-rakers. There are small cardiform teeth on the upper surface of the first four arches. There are two small groups of cardiform teeth on the heads of the third and fourth epibranchials for upper pharyngeal teeth. The lower pharyngeal teeth are in a long, narrow V shape of sharp cardiform teeth. The surface of the basibranchials has small cardiform teeth.

SALMONIDÆ.

Salmo salar (the Salmon) has thirteen short horny gill-rakers on the first cerato-hypobranchial arch, with eight on the epibranchial. The longest about a third the depth of the gill-lamina below it. They all bear teeth. There are only three minute gill-rakers on the inside of the first arch and these are placed at the end near the tongue. The outer edges of the other arches are furnished with similar but smaller gill-rakers to those of the first arch. The second arch has five minute gill-rakers on its inner side at the tongue end. The third and fourth arches have them both sides all along. The upper pharyngeal teeth consist of a very few small cardiform teeth along the upper part of the third epibranchial, and rather more and larger teeth on the upper part of the fourth epibranchial, but even there these teeth are small. In neither case are they arranged in such a dense group as is so common in other fishes. The lower pharyngeal teeth consist of, similarly, a few cardiform teeth, those nearest the œsophagus being fairly large. The tongue has teeth on it.

Thymallus vulgaris (the Grayling), an altogether fresh-water member of the family, living in rivers, has twenty-two long, thin, horny gill-rakers on the first cerato-hypobranchial arch, with thirteen on its epibranchial. All these gill-rakers are toothed. The longest is about half the length of the distance from the angle to the junction with the basibranchials. They decrease in size very quickly but very uniformly to each end from the angle. There are numerous thin horny gill-rakers on the outer edges of the other branchial arches, but none of them have gill-rakers on their inner faces. No pharyngeal teeth could be made out in the specimen examined.

Salvelinus willughbii (the Charr), from Lake Windermere, has fifteen moderately long horny gill-rakers on the first cerato-hypobranchial arch with six on its epibranchial. The longest about half the depth of the gill-lamina below it. The other arches have short gill-rakers on the outer sides only. The first and second arches show some small papillæ-like excrescences on their inner sides. The upper and lower pharyngeal teeth are very minute and cardiform. There is a small set on the heads of the third and fourth epibranchials, and on the lower pharyngeal bones they require to be looked at with a microscope to be fully realized. A row of teeth runs along the tongue to the opening of the second gill-slit.

SYMBRANCHIDÆ.

Amphipnous cuchia, an eel-like Indian fish, furnished with an air sac on each side of its neck, enabling it to breathe atmospheric air directly and so live comfortably out of water for some time. Has only three branchial arches and no gill-rakers. The upper pharyngeal teeth are in a circular group on the head of the third epibranchial arch. They are so much embedded in mucous membrane as not to be easily seen, but are quite palpable to the touch if a finger-tip is pressed against them. In a similar way cognizance can be taken of the existence of lower pharyngeal teeth.

APODES.

Anguilla vulgaris (the Common Eel) has no gill-rakers. The upper pharyngeal teeth are represented on each side by an oval patch on the head of the fourth epibranchial of minute cardiform teeth. Two long narrow slips of similar teeth form the lower pharyngeal teeth.

Conger vulgaris (the Conger Eel) has no gill-rakers. The upper pharyngeal teeth are in a triangular group on the heads of the third and fourth epibranchials, and are cardiform teeth; the group is broad at the base, narrow at the top. The lower pharyngeal teeth are similar, but set in two elongated plates.

Myrus vulgaris, from the Mediterranean, has cardiform teeth for the upper and lower pharyngeal teeth. The lower on two long broad plates, and the upper in fair-sized groups. Fig. II., 2.

Murænesox talabon (the "Koolarie" of the Tamil fishermen of Madras) has no gill-rakers. The upper pharyngeal teeth in two long narrow plates, 1·1 in. by ·3 in. of small cardiform teeth, those on the inner edges being a little larger than the others. The lower pharyngeals in two long narrow plates a little longer than those of the upper, but only two-thirds of their breadth.

MURÆNIDÆ.

Muræna helena, common in the Mediterranean and occasionally met with on the south-west coast of England, has no gill-rakers. The upper pharyngeal teeth are on a long narrow plate attached to the head of the fourth epibranchial, and are strong, sharp, cardiform teeth in a single row. The lower pharyngeal teeth are similarly strong, sharp, cardiform teeth arranged partly in a double, partly in a single row. Fig. II., 1.

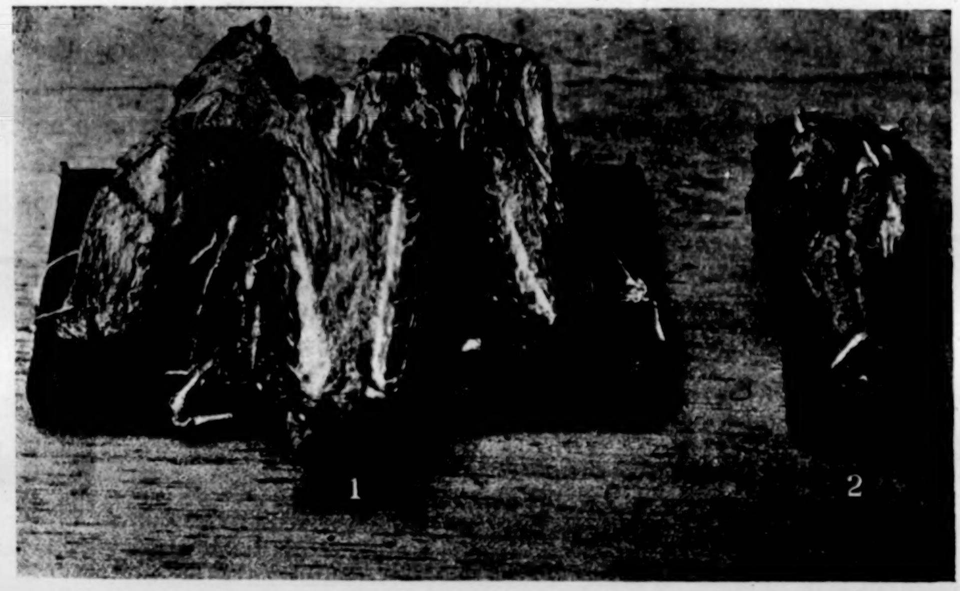

FIG. II.
1.—MURÆNA HELENA. 2.—MYRUS VULGARIS.

ESOCIDÆ.

Esox lucius (the Pike) has no gill-rakers, the surface of the cerato-hypobranchials being covered with minute teeth; a few extend also up the epibranchials. The upper pharyngeals are in two long plates, but sparsely supplied with small cardiform teeth. The lower pharyngeal teeth are in two rather broad patches of similar but smaller teeth. There is a long patch of minute teeth on the tongue and forward part of the basibranchials, also a small ovoid patch on the junction of the third and fourth hypobranchial with the basibranchials. This fish is a voracious feeder; fish, frogs, young of waterfowl, water rats, &c., form its prey, and the small number of pharyngeal teeth would seem inadequate for disposing of its food, did one not take into account the formidable amount of teeth on its palatines and vomer.

SCOPELIDÆ.

Scopelus punctatus has seventeen long horny gill-rakers on the first cerato-hypobranchial, the longest about twice the length of the gill-lamina below it; there are six gill-rakers on the epibranchial. The other branchial arches have gill-rakers on both sides, those on the outer sides being larger than those on the inner sides, but none are as long as those on the outer side of the first arch. The upper pharyngeal teeth show as two plates with cardiform teeth, with a strip of mucous membrane separating them. The lower pharyngeal teeth are very minute and very scanty.

CYPRINODONTIDÆ.

Haplochilus playfairii has twelve little horny gill-rakers on the first cerato-hypobranchial arch, with two on the epibranchial, the other arches have similar but smaller gill-rakers each side that fit alternately into one another, forming a good filter. The upper pharyngeal teeth show as two circular patches of cardiform teeth. The lower pharyngeal teeth are on two triangular plates.

Anableps tetropthalmus (the "Four eyes" of Demerara) has twenty-one horny gill-rakers on the first cerato-hypobranchial arch, the longest in length about half the depth of the gill-lamina below it. No gill-rakers on the epibranchial. There are horny gill-rakers on the inner and outer sides of the other arches which have a thickening at their roots, they fit in alternately and form a close filter. There are two patches each side, of upper pharyngeal teeth and a small narrow patch on the head of the second epibranchial. The teeth are strong cardiform teeth in the upper patch and on the head of the second epibranchial, the lower patch in its upper portion has small cardiform teeth with villiform teeth in its lower. The lower margin of the lower patch stands away from the mucous lining of the mouth. The lower pharyngeal teeth are on an arrowhead-shaped patch, the right and left halves touching closely along the middle line where there is a double row of strong conical teeth, the rest of the surface being covered with minute teeth that have black points.

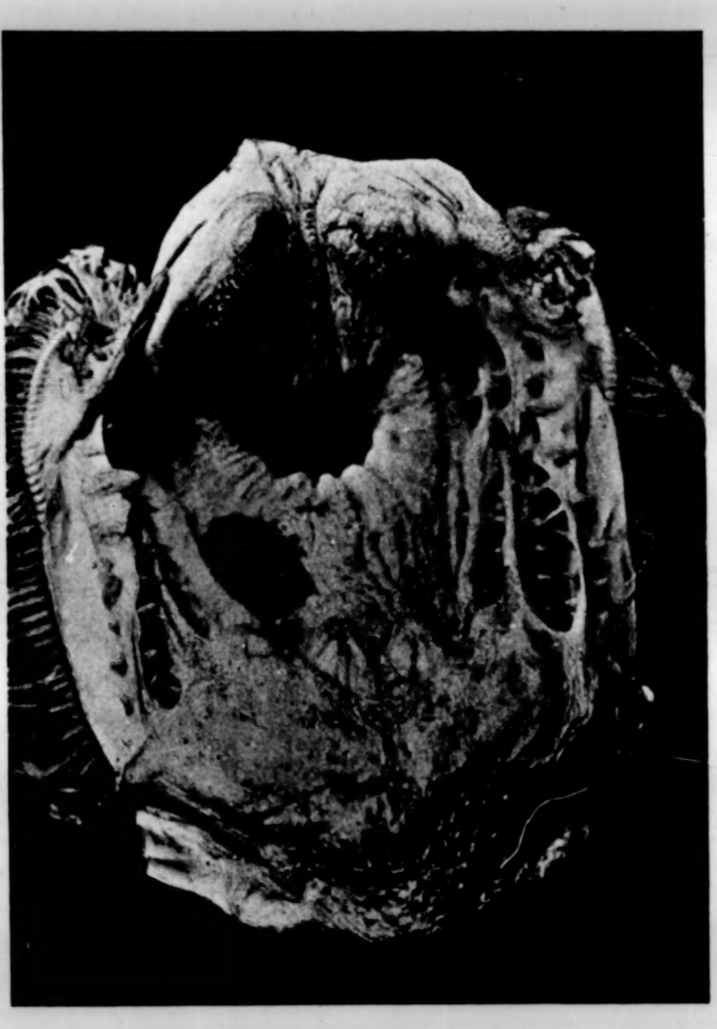

FIG. III.—CYCLOPTERUS LUMPUS.

CYCLOPTERIDÆ.

Cyclopterus lumpus (the "Lumpsucker," also called "Cock and Hen Paddle") has eight tubercular, conical-shaped, soft gill-rakers on the first cerato-hypobranchial arch that are irregularly spaced. The biggest one, at the angle, is in height about a third of the depth of the gill-lamina below it. The inner side of the first and both sides of the second and third arches have conical tubercle gill-rakers. All these tubercles are soft and distantly spaced from each other, so they do not make a close filter. There are only four branchial arches. The upper pharyngeal teeth are in a long patch, broader at the top than at the bottom; on the second and third epibranchials these teeth are cardiform. The lower pharyngeal teeth are in two small elevated lumps at the forward end of the fourth arch and very far forward from the œsophagus, and are strong cardiform teeth. (Fig. III.) A small piece of black paper was put between the two sets of the lower pharyngeal teeth, before the photograph was taken, to facilitate locating them; they are to be seen to the right and left of this little black mark.

(To be continued.)

NOTES AND QUERIES.

AVES.

Nesting of the Golden-crested Wren (Regulus cristatus).—

April 2nd, 7 a.m. A pair commenced nesting in a small Douglas fir-tree in my garden. The nest only 3 ft. 6 in. from the ground.

April 13th. Lining nest with feathers.

April 21st. First egg laid.

April 28th, 3 p.m. Eight eggs and not yet incubating.

April 29th. Clutch of nine eggs and female sitting close, until almost touched with my hand.

May 8th. Eight eggs only now in nest.

May 13th, 7 a.m. Eight eggs. 5.30 p.m. One young, seven eggs.

May 14th, 7 a.m. Three young, five eggs. 7 p.m. Five young, three eggs.

May 15th, 7 a.m. Six young, two eggs. 7 p.m. Seven young, one addled egg.

May 16th. Six young alive, one dead.

May 31st, 12 p.m. Young still in nest.

June 1st, 12 p.m. All have now left the nest.

From the above notes the building of nest takes eighteen days. Incubation fourteen to sixteen days. Young remain in nest sixteen to nineteen days.—J. Steele Elliott (Dowles Manor, Salop).

Nesting of the Reed-Warbler (Acrocephalus streperus).—On June 9th I found a Reed-Warbler's nest with four eggs, and took one for a young friend. It proved to be quite fresh, so I visited the nest again on the 12th for the chance of finding a Cuckoo's egg. The nest then looked like a new one, hardly completed, as some of the material was quite green, and I found two eggs hidden under a fresh lining; the third I could not find. The Reed-Warbler is a curious little bird in its domestic arrangements, and its nests sometimes disappear under conditions which rather convey the idea that the owners have pulled them to pieces and rebuilt them in another place.

P.S.—Referring to the above note posted a few days ago, the birds have completed the nest, which I fear I rather pulled about in getting at the covered-up eggs, and the hen is sitting on four eggs. I have never seen a case like it before.—Julian G. Tuck (Tostock Rectory, Bury St. Edmunds).

Curious Nesting-sites of Chaffinch and Blue Tit.—On May 15th last I found a nest and eggs of the Chaffinch in Turvey Park, built on the side of a wheat-straw stack in the rick-yard. The nest had been woven around a number of the straws, which kept it securely in position. The site was purely a matter of choice as there are plenty of trees and bushes in the vicinity. At Southill Park, also in Bedfordshire, on June 2nd, noticing a Blue Tit leave a Wren's nest that had been built in the fork of an elderberry bush alongside the ride of a plantation, I found the former species in occupation and sitting on a small clutch of eggs. Evidently it had deserted some previous nesting-site, as no materials appeared to have been added to the nest whatever, not even for a lining.—J. STEELE ELLIOTT (Dowles Manor, Salop).

Starling and Mouse.—On June 19th, while walking along a footpath dividing a ploughed field from a wood, I noticed, a short distance ahead, a Starling acting in a strange manner. It had possession of some living object of comparatively large size, with which it seemed at a loss to know how to deal. The bird kept taking this up in its beak, putting it down again, and pecking savagely at it, and was apparently in a very excited state. It seemed very loth to leave its prize as I approached, and tried to carry it off, but, finding it too big and heavy, was obliged reluctantly to fly away without it. The object referred to proved to be a young Mouse (*Mus sylvaticus*), at least a fortnight and more probably three weeks old, it being already covered with short fur. In spite of this rough treatment, the little animal appeared to be very little if at all hurt. This is the only instance I have met with of a Starling attacking a mammal.—G. T. ROPE.

Food of the Little Owl (Athene noctua).—To show the diversified prey of this species of Owl, I give a list of the contents taken from a nest in a pollard willow tree at Turvey, Bedfordshire, on June 28th last; the nest contained but one young one:—Six adult and two half-grown Common Field Voles (*Microtus agrestis*), four of which were more or less eaten, the head portion of these having been devoured first; one adult and three young Common Shrews (*Sorex araneus*), one partly eaten; five Frogs, which included the hind legs only of three adults with two smaller Frogs; remains of a fully-grown Blackbird and one immature Thrush, probably taken from a nest; with a number of elytra of beetles. In a hollow of the next willow tree was the headless body of a fully-grown Thrush.—W. J. ELLIOTT (Dowles Manor, Bewdley).

The Redshank in Norfolk Marshes.—Writing respecting the increase of the Redshank (*Totanus calidris*) in our marshes, alluded to in the "Ornithological Report for Norfolk" (*ante*, p. 169), Mr. J. Vincent mentions having this spring found thirty-five nests (of which the earliest was on April 15th), which is certainly a very satisfactory state of things. He has never in his long experience found a nest with eggs earlier than April 7th, and consequently expresses some incredulity as to young Redshanks being able to fly by April 25th, as reported last year (*ante*, p. 169) on the authority of Mr. Chasen. Certainly in that case they would have had to be hatched about April 1st, which is unprecedentedly early. May I take this opportunity of correcting my mistake pointed out by the Duchess of Bedford (*ante*, p. 175), where, by an oversight, the name Lesser Tern is made to do duty for a Common Tern feeding its young one. From the principal Norfolk breeding-place of these birds also a very good report came to hand from Sir T. Digby Pigott at the beginning of the month (June 5th), and it is hoped the young will not again die for want of Whitebait as they did last year.—J. H. GURNEY (Keswick Hall, Norwich).

Destruction of Vermin in Bedfordshire.—It is evident that, with the continued increase in game preserving, the destruction of so-called "vermin" will considerably alter the present status of many species, and it would be of interest if "vermin accounts" from any considerable areas of shooting could be published. The following list is from an estate of 2000 acres in the parish of Turvey, Bedfordshire. Previous to 1912 no strict preserving had been carried on for several years, and this accounts for the unusual number of vermin accounted for on so small an acreage:—

1912.

	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	Total.
Stoats			2	5	1	9	7	4				4	32
Weasels ..		7	13	5	7	13	20	12	3	3		2	85
Cats......			5	4	2			5				2	18
Rats......		5		13		7	12	9	1	4		13	64
Hedgehogs		1		3	5	1	3	3				1	17
Hawks ..			1	1	4	1	2	1	1	1			12
Owls			4	8	2	2	11	7	3	4			41
Crows				2	3								5
Rooks				4									4
Jackdaws .				1	6	2	1						10
Magpies ..				13	9	10	3						35
Jays......		7	3	7		7							24
													347

1913.

	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	Total.
Stoats		1					3	2	1	1	2	4	14
Weasels ..		3	5	2	5	5	6	11	13	7	3	1	61
Cats......		5	8	5	3	1	1			2	2		27
Rats......		18	6	8	6	3	3	3	7	9	3	4	70
Hedgehogs				3	2		1	2	3	11	4		26
Hawks....				1				1	2				4
Owls*			2				2	4	5	8	3		24
Crows													
Rooks													
Jackdaws .					2								2
Magpies ..			3		3	3		1					10
Jays......				1	1	4							6
													244

In 1913 an assistant keeper was also employed, but a detailed list was not retained, but an additional one hundred and sixty-two "vermin" were destroyed by him. The considerable destruction of Owls is regrettable, probably three-fourths of those killed being Little Owls. Tawny Owls, Barn Owls, and Long-eared Owls all met a similar fate. Of the Hawks, the Kestrel and Sparrow-Hawks would be about equal in the numbers destroyed. These accounts I know are often difficult to procure, as it is very seldom on shooting estates that the law is strictly adhered to, and many protected species are wilfully slaughtered, and I regret to think that that abominable and illegal means of destruction, the "pole-trap," is still freely used.—J. Steele Elliott (Dowles Manor, Salop).

Notes on Nest-boxes.—The Tawny Owls which have nested in our church-tower for several years past deserted the place this year after the second egg was laid, but I have every reason to believe that they hatched off successfully on the property adjoining the churchyard, where they are strictly protected. Our own pair occupied an old cask in an elm close to the house, and brought off three young ones. One of these when in the down was much more yellow than the others. An old kettle was twice occupied by Robins, but the second brood were destroyed by a Rat or other marauder. In our boxes we had the Great Tit, Blue Tit, Coal Tit (one), Marsh Tit (one, and only the second nest of this species we ever had in a box), Nuthatch (one), Tree-Sparrow (many), Starling, and Stock-Dove. The last-named bird seems on the increase here, as they have occupied four boxes. One sitting bird I think came to grief, as I found a great many feathers in the box one day, and two deserted eggs about half incubated. Possibly a Tawny Owl was the aggressor, but our pair here have always been very peaceable with human beings. When spring-cleaning the boxes early in the year, I found in one which had been occupied two or three times over by Stock-Doves last year enough "guano" to give a dressing to a bed of aquilegias, which did remarkably well on it. On June 10th I was looking at a box and heard a humming inside, which I put down to a bee. However, the tenant proved to be a large queen Hornet, which had begun a nest there, and made it up to about the size of an oyster-shell. It is needless to add that both nest and owner were promptly destroyed. To find a Bat in a box is not uncommon, and one which we found one evening was a source of great delight to some young friends who had never seen one in the hand before. He was, I think, a Pipistrelle; we put him back in his retreat, and I believe I found him again in another box about a month later. We used often to find Great Bats in boxes, but have not seen one for some years. This year we had hoped to add the Jackdaw to our list of box-breeders, but the two pairs which nested about the place both preferred hollow beeches. As for several years past, Redstarts and Wrynecks have been absent; Swallows and Martins more abundant than usual.—JULIAN G. TUCK (Tostock Rectory, Bury St. Edmunds, Suffolk).

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* I have a note, May 4th, 1913, counting nineteen Little Owls and twenty-six Magpies, all recently killed, hanging on one of the "keeper's gibbets," and on a later date four Tawny Owls and one Barn-Owl hanging alongside the ride opposite a pole-trap.

NOTICES OF NEW BOOKS.

Field-Studies of some Rarer British Birds. By JOHN WALPOLE-BOND. Witherby & Co.

THIS is one of the most interesting of all recent books on the birds of the British Archipelago. As we read these pages we are with live birds themselves in their natural environment, the intricacies of a revised nomenclature are quite unimportant, classification is forgotten, we are not out for skins but for facts in avian economy, eggs are observed—not clutched. Some of the records, of course, are not unfamiliar to field ornithologists, but very many are quite original and hitherto unrecorded, while *en masse* they will greatly encourage and enrich the annals of "bird-watching." The suggestion that Crossbills may in some instances be triple-brooded will be new to many, and so also will be the fact "that in *hard* springs (or rather late winters) Ravens are apt to be forward with their domestic affairs, in *open* seasons, backward."

Seventeen birds afford the material for eighteen chapters, and we hope that Mr. Walpole-Bond will publish some more of his notes (which he undoubtedly possesses) on other British birds.

The Naturalist at the Sea-Shore. By RICHARD ELMHIRST, F.L.S., Superintendent of the Marine Biological Station, Millport. Adam & Charles Black.

THIS is a recent volume of the series of books published under the comprehensive title of "Peeps at Nature," and is written by one who, as superintendent of a Scottish marine biological station, is master of his subject. Such a book is very opportune at this season of the year, and should be in vogue among seaside visitors who feel an interest in the plant and animal life of the coast line they may frequent. These pages contain a great deal of zoological information, which is not only

realiable but may be readily understood by the ordinary beach reader, and contains thirty-seven illustrations, of which eight are coloured full-page plates. At the small price charged for the volumes of this series nothing more can well be desired or expected.

EDITORIAL GLEANINGS.

"ENQUIRIES made by Mr. Dudley Le Souef, Director of the Zoological Gardens in Melbourne, in regard to Eels in New Zealand have brought several notes from correspondents. Mr. Le Souef expressed a wish that those who accused Eels of eating Trout had found Trout inside the Eels caught, as otherwise the evidence against the Eels could hardly be regarded as conclusive. Mr. J. Finlay, of Tokaora, Hawera, writing on March 17th, now supplies the evidence Mr. Le Souef asks for. 'My farm,' Mr. Finlay states, 'has a frontage of about fifty chains on to the beach in the South Taranaki Bight, and is close to the mouth of the Waingongoro River. Complaints reached the custodian of the South Taranaki fishponds that the lower reaches of this river were nearly depleted of Trout, although only a few years previously large quantities of Trout had been liberated in it. The custodian, Mr. Moorehouse, came to my house one forenoon with an Eel he had caught. It was 4 ft. 9 in. long and weighed 10½ lb. When we opened it we found a whole Trout, quite fresh, which turned the scale at 1 lb. 15 oz. The Eel also contained about half another Trout, partly digested, which probably would have weighed about 2 lb. Each Trout had been swallowed head first.' Mr. Finlay adds that it is admitted on the South Taranaki coast that large Eels swallow Trout. About the same time two men who were fencing for him fished for Eels in the same river one night. They caught twenty-six Eels, weighing from 2 lb. to 5 lb. each. The Eels were opened in order to discover if small Eels as well as large ones eat Trout, but no Trout were found in any of them.

"An adjoining farmer constructed a farm about seven acres in area. It was stocked with young Trout, which thrived for a time, but then gradually disappeared. One day the farmer's sons caught in the dam an Eel that weighed 32 lb. As it was given to Maoris, Mr.

Finlay was not able to discover what was inside it. The evidence against it, therefore, is inconclusive. The dam was stocked with Perch, which were very successful. Later on, as land in the district, being used for dairying, became very valuable, reaching from £70 to £80 an acre, the water was let out of the dam, and the custodian of the local hatcheries was able to rescue a large number of yearlings and large fish, up to 6 lb., for stripping. But when the water was released not a single Eel was caught in the dam or seen in it. If Eels eat Trout, Mr. Finlay asks, do Perch eat Eels?

"Further evidence is supplied by Mr. A. W. Babbage, of Hauturu, Kawhia. He states that about seven years ago an Eel weighing 7 lb. was caught in the Kaimango stream by Mr. F. Pearson. Inside there were two Trout, one 10 in. long and the other 12 in. The smaller one was slightly digested, but the larger one was quite fresh, and, evidently, had been swallowed only a short time previously. Mr. H. P. Washbourn, of Port Nelson, has entered with great vigour into the discussion in regard to the breeding of Eels. He has sent to Maoris and Europeans in the dominion a circular containing a number of questions. All the men to whom the circular has been addressed have had many opportunities for observing the habits of Eels. Mr. Washbourn feels that their replies will be at least interesting records, and he believes that they will show that the lives and habits of New Zealand Eels are not settled by the lives and habits of Eels in the Old Country."—'Lyttleton Times,' April, 1914.

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